

Clim. Past Discuss., author comment AC9  
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## Reply on RC3

Fabrizio Marra et al.

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Author comment on "Terrestrial records of glacial terminations V and IV and insights on deglacial mechanisms" by Fabrizio Marra et al., Clim. Past Discuss.,  
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I consider the outcome of the review process to be deeply unfair.

The Editor based his judgment on two reviewers whose responses were sort of a "past-and-copy" of incorrect comments based on misleading claims intended solely to have the work rejected. The first one also had the bad taste to close his acrimonious review with "best wishes". Unfortunately, I know these two Italian sedimentologists who have not the heart to sign their reviews.

The Editor, after the open discussion was extended twice due to not being able to find competent reviewers, took another two months (!) just to reiterate the two reviewers' objections. I think that in the replies we had explained in depth the inconsistency of their statements and their total irrelevance to the content of the work.

In reiterating the remarks of the reviewers, without ever entering the specific context of the work, the Editor has shown, in my opinion, his inability to understand and to discuss the actual subject of the submitted work and opted for a shortcut: to share the bad faith and prejudice of the reviewers totally inadequate he had chosen.

The objections at points (1), (2), (3) are ridiculous and somewhat offensive. The editor states that there is no "decent" level of stestigraphic decription, no justification of correlation made across the basin and of deviations (?) from published interpretatioin/description (which ones?).

And which are these "alternative" descriptions, correlations and interpretations? Those that the reviewers have proposed based on the features of the Holocene Liri-Lacustrine basin, when we are describing those of the 600-350 ka interval, or those that they didn't even mention?

The statement: "the lithostratigraphic descriptions are not sufficient for readers to evaluate the significance of the coarse units with respect to a region deglaciation signal" is simply false. What is the difficulty in understanding the difference in water transport energy between a suite of silty-clay lacustrine sediments and the intercalated, 3-4 m thick, 10-20 km wide, beds of coarse gravel (diameter of pebbles >5-10 cm)?

As we have explained in our replies, the subject of the work is not sedimentology, but the direct correlation, demonstrated by 40Ar / 39Ar dating, between the deposition of

extensive gravel beds in the catchment basins of the major rivers of central Italy and the deglacial process at global scale.

This has been appreciated by the paleoclimate community, as the nearly 1,000 reads demonstrate, but is evidently irrelevant for the Editor of the journal, who prefers that the work will be published in another venue.

Fabrizio Marra